DOCUMENT RESUME

ED 265 532 CS 209 261

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TITLE Protocols, Retrospective Reports, and the Stream of Consciousness. CDC Technical Report No. 8.
INSTITUTION Carnegie-Mellon Univ., Pittsburgh, PA. Communications Design Center.
REPORT NO CDC-TR-8
PUB DATE Mar 85
NOTE 22p.
PUB TYPE Viewpoints (120) -- Information Analyses (070)

EDRS PRICE MF01/PC01 Plus Postage.
DESCRIPTORS Cognitive Processes; *Educational Theories; *Protocol Analysis; Research Methodology; Writing Instruction; *Writing Processes; *Writing Research

ABSTRACT

Noting that recent issues of several scholarly journals have contained criticism of protocol analysis and the use of verbal reports in the analysis of the writing process, this paper examines some of the charges made and then explores the implications of the criticisms. Specific charges addressed in the paper include the following: (1) protocols and the transcripts that represent them seriously falsify what is happening in the mind of the writer giving the protocol; (2) protocols are works of fiction; (3) cognitive processes are not visible enough to be examined scientifically; (4) the presence of the person arranging for the protocol and of the tape recorder, as well as the nature of the protocol session, distort the cognitive processes of the writer; and (5) protocol analysis was rejected as unscientific by psychologists many years ago. After making the case that a closer look at one particular criticism shows how it indicates a misunderstanding by the critics of the true nature of protocol analysis, the paper cites works by cognitive psychologists and others who have examined the writing process from this perspective to demonstrate how valuable protocol analysis and similar techniques can be. Finally, the paper deals with the question of whether protocol analysis is scientific and with the inconsistencies present in many criticisms of the technique. (FL)

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CDC Technical Report No. 8

Protocols, Retrospective Reports, and the Stream of Consciousness

Erwin R. Steinberg

March 1985

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Protocols, Retrospective Reports, and the Stream of Consciousness

Erwin R. Steinberg

Attempts to use the methods of cognitive psychology to analyze the writing process have met with serious criticism in College English and College Composition and Communication. Many of the attacks have been aimed at protocol analysis and the use of verbal reports as data. I should like to examine several of the major charges and then go on to examine the implications of the criticism.[1]

1. Protocols and the transcripts which represent them seriously falsify what is happening in the mind of the writer giving the protocol. "[T]he thoughts that run through our heads while we are writing (or, in fact, while engaged in almost any activity) are diffuse, highly branched, visual as well as verbal" (Marilyn Cooper and Michael Holzman, "Talking About Protocols, College Composition and Communication, 34 [October 1983], pp. 284-293).

Critics here confuse the stream of consciousness, where thought is relatively unconstrained, and problem solving, where thought is much more constrained and focused. The actual stream of consciousness "contains not only language, but a whole variety of other items: spatial, temporal, and purposive orientation; kinesthetic elements and related data; vocal patterns, etc. A stream of consciousness could also contain memory in the form of visual and other sensory images. All of this gives the effect of flowing in three dimensions of space through time and of functioning at several levels of abstraction" (Erwin R. Steinberg, "The Stream-of-Consciousness Technique: Simulating a Stream of Concurrencies," Imagination, Cognition and Personality, 2 [1982-83], pp. 241-249). Thus, in the actual stream of consciousness there are many activities going on in parallel at the same time: the stream of consciousness is multidimensional.

However, spoken and written language, in which protocols must be articulated, are unidimensional: linear. Thus, critics claim, a protocol distorts. Such an argument represents fairly the dilemma of the stream-of-consciousness novelist who can only simulate the actual stream of consciousness (Steinberg, 1982-83, p. 243; Erwin R. Steinberg, "Stream of

Consciousness," in Encyclopedia of World Literature in the Twentieth Century, vol. 4, ed. Leonard S. Klein [New York: Frederick Ungar, 1984], pp. 347-350). It is not a valid argument, however, against protocols.

For when one's attention is directed at solving a problem, as it is during a protocol, consciousness is much more tightly focused:

> Our model assumes that *only information in focal attention* can be verbalized. In our model, as in most theories of the structure of the human information-processing system, a distinction is made between fast automatic processes that are not necessarily conscious (and that are often thought to proceed in parallel) and the slow serial processes that are executed under cognitive control--a distinction, that is, between preattentive and focally attended processes . . . perceptual and cognitive processes . . . and automatic and cognitively controlled processes . . . . (K. Anders Ericsson and Herbert A. Simon, "Verbal Reports as Data," Psychological Review, 87 [May 1980], p. 235)

As Simon explains elsewhere:

> [T]he kind of mental activity that requires at least a modicum of attention is essentially serial, for it must have access to the very limited capacity of short-term memory for its inputs and outputs. (Herbert A. Simon, Models of Thought [New Haven: Yale University Press, 1979], p. 4)

By "serial" activity Simon means focused mental processes, which are different from the relatively unfocused stream of consciousness or automatic cognitive information processing, where many activities go on at the same time--i.e., in parallel.

All of us have experienced such focussing of attention. It is a common experience for academics to narrow one's attention so tightly on the problem at hand as to "tune out" the radio, street noises, changes in temperature, even calls to the dinner table. The "absent-minded professor" is one who carries this tight focusing of attention to extremes.

2. Protocols are works of fiction. Cooper and Holzman complain about the protocols presented by Flower and Hayes: "Their introspectors notice virtually nothing other than that which is to the point. Do these people never fantasize about, say, lunch? Or were they instructed never to mention such things? Or were the transcriptions of the protocols edited? Do protocols 'capture a detailed record' [Linda Flower and John R. Hayes, "A Cognitive Process Theory of Writing," College Composition and Communication, 32 (December 1981), p. 368], or invent one, one as literary in its own way as those of [Virginia] Woolf and other writers?" (Cooper and Holzman, p. 290).

I have given several protocols in the process of writing or revising. What I said aloud did not represent everything that went through my mind at the time: my awareness of a disturbance in the hall, for example, or of a pile of papers at my left elbow. My attention, nevertheless, remained firmly focused on the task at hand. (I did not fantasize about lunch--or anything else.) And what I said aloud that was captured by the tape recorder adequately represented that focused attention and the cognitive processes involved in my writing or revision.

The evidence is that protocols reflect cognitive processes adequately enough to build representative problem-solving models from them--models, for example, on the basis of which computers have been programmed to be very good chess and backgammon players. Those who disagree should come up with evidence to the contrary. It is not enough to ask superciliously whether "these people never fantasize, say about lunch." Playing for a snicker may win a few readers in a short run, but it is not evidence useful in evaluating protocol analysis; and it particularly ill-becomes critics who presume to scientific understanding by criticizing others for not meeting "Turing's test for validity" and for not being "ecologically valid" (Cooper and Holzman, 287, 291).

3. Cognitive processes are not visible enough to be examined scientifically. Connors quotes psychologist James Deese's statement that "the origins of a significant portion of cognitive events are simply unavailable for experimentation in the classical sense" (Robert J. Connors, "Composition Studies and Science," College English, 45[January 1983], p. 12; James Deese, "Behavior and Fact," American Psychologist, 24[May 1969], p. 518). Perhaps the origins are unavailable (Connors refers immediately after this quotation from Deese to the "over ten billion neurological units, each with connections which may lead to as many as 25,000 other; Connors, pp. 12-13). How is that relevant? It is the cognitive events themselves that we are after. Furthermore, by "in the classical sense," Deese means "the classical experimental model" (Deese, p. 518), "which allows phenomena to be disembodied from their contexts and analyzed by being ordered to formal and mathematical systems" (Connors, p. 6). Deese does not say that cognitive events are not available by other means. Indeed, he says explicitly, "That does not mean that we cannot apply the tools of science [to cognitive events], only that we should consider whether the particular tools being used serve any genuinely useful purpose" (Deese, p.518). As I have

explained above, a protocol adequately represents focused mental activity during problem solving.

4. The presence of the person arranging for the protocol and of the tape recorder and the very nature of the protocol session distort the cognitive processes of the writer giving the protocol. During a protocol session, "The recorder becomes part of the process being studied. And that skews the results, for all results reflect the recorder's presence as a factor" (Ralph Voss, "Janet Emig's The Composing Process of Twelfth Graders: A Reassessment," College Composition and Communication, 34[October 1983]. Connors claims "that by experimentally ordering behavior we must change it" (Connors, p. 12).

Similar problems have not prevented progress in science and social science. Thus, the uncertainty principle,

> which was formulated by Werner Heisenberg (in 1927), and which plays a fundamental role in quantum mechanics . . . states that it is impossible to specify or determine simultaneously both the position and velocity of a particle as accurately as is wished. It is, to be sure, possible to fix either of these quantities as precisely as desired, but only at a price, for the greater precision in one, the greater the inevitable lack of definiteness in the other. (J. H. Van Vleck, "The Uncertainty Principle," Encyclopaedia Britannica, v.22 [Chicago: Encyclopaedia Britannica, 1961], p. 680)

As Max Plank explains, the problem is caused by the observation:

> We can determine the position of a moving electron only if we can see it and in order to see it we must illuminate it, i.e. we must allow light to fall on it. The rays falling on it impinge upon the electron and thus alter its velocity in a way which is impossible to calculate. (Max Plank, The Philosophy of Physics, trans. W. H. Johnston [London: Allen and Unwin, 1936], p. 58).

Einstein concluded, therefore, that

> Heisenberg has convincingly shown, from an empirical point of view, any decision as to a rigorously deterministic structure of nature is definitely ruled out, because of the atomistic structure of our experimental apparatus. Thus it is probably out of the question that any future knowledge can compel physics again to relinquish our present statistical theoretical foundation in favor of a deterministic one which would deal directly with physical reality. (Albert Einstein, Out of My Later Years [New York: Philosophical Library, 1950], pp. 109-110)

The Encyclopaedia Britannica article puts the matter more simply: "The safest conclusion is perhaps that man remain humble in the face of nature since there are inherent limitations to the precision with which he can observe the latter" (Van Vleck, p. 681). Protocols may not reflect precisely what mental activity goes on during problem solving; but in thirty-five years they have enabled cognitive psychologists and specialists in artificial intelligence to model problem-

solving methods quite carefully, as a considerable body of literature demonstrates. (See, for example, Allen Newell and Herbert A. Simon, Human Problem Solving [Englewood Cliffs, NJ: Prentice Hall, 1972]).

Furthermore, cognitive psychologists do not claim that protocols give us the cognitive processes whole. Rather, "With the direction to verbalize, a direct trace is obtained . . . and hence an indirect one of the internal stages of the cognitive process" (Ericsson and Simon, 1980, p. 220). Physical scientists regularly use such tracing methods when exploring phenomena they can not see directly.

Nor does speaking aloud ("concurrent verbalizing") while solving a problem at the level required in giving a protocol during writing distort the cognitive processes themselves enough to cast doubt on the usefulness of the traces that such a protocol provides:

> When the subjects articulate information directly that is already available to them, [our] model predicts that thinking aloud will not change the course and structure of the cognitive processes. Nor will verbalization under these conditions slow down these processes.
>
> When the information being processed in order to perform the main task is not verbal or propositional, the model predicts that the performance may be slowed down, and the verbalization may be incomplete but that the course and structure of the task-performance process will remain largely unchanged. (Ericsson and Simon, 1980, p. 227; see also pp. 228-229)

It is also relevant that participant observers in social science investigations are much more intrusive than are observers during a protocol session. Sociologists, anthropologists, and of course, pollsters intrude regularly into the activities they observe and record. Are we to deny validity to all their research?

5. Protocol analysis was rejected as unscientific by psychologists many years ago. "[Wilhelm] Wundt [a late nineteenth-century psychologist who pioneered introspection] found that [his introspective] investigations were best conducted through the use of highly trained 'introspectors,' research assistants who were able to verbalize their mental states . . . . [T]he Flower/Hayes . . . introspectors are also trained, or instructed, and do their work under quite special conditions" (Cooper and Holzman, p. 289).

Cooper and Holzman are right about Wundt's investigators. Wundt insisted on many hours of training before he used their reports. But Wundt and his followers were not using protocol

analysis. Protocol taking is much simpler than Wundt's introspective techniques. Persons giving protocols are not "trained." Nor do they "do their work under quite special conditions." A writer from whom a protocol is to be taken is simply seated at a desk in an office and given an assignment. He or she may ask about the assignment, just as a student may ask a professor about an assignment or as an engineer may ask a supervisor about one. The observer then gives a short set of instructions, seldom more elaborate than the following:

1. Say whatever's on your mind. Don't hold back hunches, guesses, wild ideas, images, intentions.

2. Speak as continuously as possible. Say something at least once every five seconds, even if only, "I'm drawing a blank."

3. Speak audibly. Watch out for your voice dropping as you become involved.

4. Speak as telegraphically as you please. Don't worry about complete sentences and eloquence.

5. Don't over explain or justify. Analyze no more than you would normally.

6. Don't elaborate past events. Get into the pattern of saying what you're thinking now, not of thinking for a while and then describing your thoughts.

> Simply informing people of these six principles helps considerably. It's also useful to provide five minutes or so of practice on some light task unrelated to the experiment. People can be reassured by telling them about the strong warm-up effect that almost always occurs. While the first minute or two of thinking aloud may feel awkward, as one becomes used to the speaking and absorbed in the activity, the reporting becomes more and more fluent. (D. N. Perkins, The Mind's Best Work [Cambridge: Harvard University Press, 1981], pp. 33-34)

The observer sits to one side and on occasion, if necessary, with a simple phrase, encourages the writer to follow rules 2, 3, and 5: "What are you thinking?" or "Please speak a little louder" or "Just report what you are thinking." Some observers prefer to sit out of sight. Others leave the room when they are satisfied that the protocol is going well. In any event, the procedure is radically different from Wundt's; and to damn protocol analysis with criticism levelled against Wundt's introspection procedure is to demonstrate lack of knowledge of both.

As Ericsson and Simon declare:

> This state of affairs [using criticism of introspection against protocol analysis] is wholly unsatisfactory if we are to make rapid and continuing progress in understanding human cognitive processes. First, no clear guidelines are provided to distinguish illegitimate "introspection" from the numerous forms of verbal output (see the earlier examples) that are routinely treated as hard data, such as passing the chain-and-transit test . . . . Second, no distinction is made between

> such diverse forms of verbalization as "thinking aloud" protocols, retrospective responses to specific probes, and the classical introspective reports of trained observers. All are jointly and loosely condemned as "introspection." (Ericsson and Simon, 1960, p. 216)

I shall return to this problem below.

How little some of the vociferous critics of protocol analysis understand the procedure is made clear in one critical article I have referred to when the authors offer as a "protocol" the opening passage from Virginia Woolf's essay on Herman Melville:

> Somewhere upon the horizon of the mind, not recognizable yet in existence, "Typee" and "Omoo" together with the name of Herman Melville, float in company. But since Herman Melville is apt to become Whyte Melville or Herman Merivale and "Omoo" for some less obvious reason connects itself with the adventures of an imaginary bushranger who is liable to turn jockey and then play a part in the drama of "Uncle Tom's Cabin," it is evident that a mist, due to ignorance or the lapse of time, must have descended upon those far distant regions . . . . Waves are breaking; there is a rough white frill of surf; and how to describe it one does not know, but there is, simultaneously, a sense of palm trees, yellow limbs, and coral beneath clear water. (Cooper and Holzman, p. 290)

Note that something has been left out (. . . .). Here is the paragraph as it actually appears in the essay:

> Somewhere upon the horizon of the mind, not recognizable yet in existence, "Typee" and "Omoo" together with the name of Herman Melville, float in company. But since Herman Melville is apt to become Whyte Melville or Herman Merivale and "Omoo" for some less obvious reason connects itself with the adventures of an imaginary bushranger who is liable to turn jockey and then play a part in the drama of "Uncle Tom's Cabin," it is evident that a mist, due to ignorance or the lapse of time, must have descended upon those far distant regions. *Ignorance we do not scruple to admit; the lapse of time, since the first of August marks the centenary of Melville's birth, is undeniable; but this haziness may spring in part from a little seed dropped years ago by the books themselves. Was not some one talking about the South Seas? "Typee," they said, was in their opinion the best account ever written of--something or other. Memory has dropped that half of the sentence, and then, as memory will, has drawn a great blue line and a yellow beach.* Waves are breaking; there is a rough white frill of surf; and how to describe it one does not know, but there is, simultaneously, a sense of palm trees, yellow limbs, and coral beneath clear water. This blundering brushwork of memory has been corrected since by Stevenson, Gauguin, Rupert Brooke and many others. Yet, in some important respects, Herman Melville, with his "Typee" and "Omoo" and his ineradicable air of the early forties, has done the business better than the more sophisticated artists of our own day. (Virginia Woolf, "Herman Melville," Books and Portraits [New York: Harcourt Brace Jovanovich, 1977], p. 80; italics added to show passage left out.)

I would make several comments:

1. The critics who offered the truncated version of the Woolf passage are the same ones

who asked whether the Flower/Hayes "transcriptions of the protocols [were] edited." The Flower/Hayes transcriptions were not edited, but these critics did edit the Woolf paragraph in order to offer it as a "protocol." (They had the grace to use the word in quotation marks.)

2. This may be a "literary introspection" and a "meditation," as they characterize it (p. 290) but it is not a protocol (with or without quotation marks). Woolf in that paragraph is not saying "whatever's on [her] mind" as she writes, nor is she "speaking continuously" (Perkins, p. 33). The paragraph is an artifact, a conscious--no, a self-conscious--creation, not a spontaneous verbalization. Woolf thought about it before she wrote it, pondered it as she wrote it, edited it after she wrote it--perhaps even showed it to her husband after she wrote it and made changes as a result. Anyone who thinks that Virginia Woolf's published writings were as spontaneous as the offering of this paragraph as a "protocol" suggests need only examine any of the Woolf manuscripts in the Berg Collection of the New York Public Library to learn otherwise. She always reworked her writing thoroughly in draft. Furthermore, protocols are spoken aloud into tape recorders and then transcribed. Virginia Woolf did not dictate; she wrote in longhand. It is hard enough to keep up with one's thoughts when speaking them aloud. It is impossible to do so when writing.

3. Finally, those who offer this paragraph by Woolf as a "protocol" call it "Her description of the way her mind wanders while writing" (Cooper and Holzman, p. 290). First, she may be describing how her mind wanders while she is writing. She is not reporting here her thoughts as she writes. The paragraph is a highly stylized, carefully edited retrospective report. Secondly, she is indeed describing, not thinking aloud. That is exactly what brought the work of Wilhelm Wundt and his introspectionist colleagues into question.

The attack against the introspectionists was led by J. B. Watson, the behaviorist, in 1913. But

> Watson made a clear distinction between analytical classical introspection, verbal questioning of a subject, and thinking aloud. His views on the veridicality of the latter kind of verbal report were quite different from his views on the first two . . . and to give evidence on this point, Watson . . . demonstrated that thinking could be made overt.
>
> > The present writer has often felt that a good deal more can be learned about the psychology of thinking by making subjects think aloud about definite problems, than by trusting to the unscientific method of

> introspection. (J. B. Watson, "Is Thinking Merely the Action of Language Mechanisms?", British Journal of Psychology, 11[October 1920], p. 91)

> After presenting the first documented analysis of thinking-aloud activity, Watson (1920) summarizes his arguments for the opinion just quoted--that the overt verbalizations in TA [thinking aloud] correspond to the normally covert thought activity--by making reference to observations from numerous individuals thinking aloud while working problems. Watson was quite clear about distinctions among modes of verbalization that have since become muddled. These distinctions were also quite apparent to the Gestalt successors of classical introspectionism. In their phenomenological observations, naive subjects were used, and the subjects were allowed to give their own spontaneous descriptions in their own language. (K. Anders Ericsson and Herbert A. Simon, Protocol Analysis, Verbal Reports as Data [Cambridge: MIT Press, 1984], pp. 58-59)

The critics who equate Wundt's introspective techniques with protocol analysis continue to muddle. They should read the history of psychology much more carefully.

There are many, many other misunderstandings and misrepresentations that we might pursue. For example, "descriptive studies" like protocol analysis (and the work of Flower and Hayes) are not "Rooted in the practice of clinical psychology" (Connors, pp. 8, 10). Anyone who thinks they are should read the "Historical Addendum" (pp. 873-889) and the "Bibliography" (pp. 891-901) in Human Problem Solving by Allen Newell and Herbert A. Simon (Englewood Cliffs, NJ: Prentice-Hall, 1972), two researchers who have been in the forefront of cognitive psychology since it began to flower in the years after World War II.

Nor is Allen Newell, who, as Connors (p. 13) explains, is "one of the developers of the technique of protocol analysis," quite as "rueful" about the state of information processing psychology as the quotation which Connors offers suggests (p. 13). That quotation represents only half of Newell's thinking. Newell explains at the beginning of the paper from which Connors quotes, "[The other] Half of me is quite content and clear on where we are going"; and he concludes that paper, "Maybe all is well . . . , and when we arrive in 1992 (the retirement date I pick might as well be my own) we will have homed in to the essential structure of the mind" (Allen Newell, "You Can't Play 20 Questions with Nature and Win," in Visual Information Processing, ed. William G. Chase [New York: Academic Press, 1973], pp. 283-306).

In the last few years we have learned many things about the process of writing by process-tracing techniques like protocol analysis. Here, for example, is a paragraph from a recent paper

by Scardemalia and Bereiter that indicates how profitable protocol analysis and other process-tracing techniques borrowed from cognitive psychology are in describing "knowledge telling," the process of "reducing writing assignments to topics, then telling what one knows about the topic":

> The knowledge telling strategy is inferred from several findings: (a) Thinking-aloud protocols of young writers consist almost entirely of content generation, with no evidence of conscious goal-directed planning . . . ; (b) Novice writers tend to present information in the order of its acquisition . . . ; (c) When stuck, novice writers tend to re-examine the assignment for cues rather than examine their goals, as experts do . . . ; (d) Students texts lack internal constraint, suggesting a forward-acting or additive approach to text generation . . . ; (e) Children's ability to honor structural constraints is shown by their ability to compose essays using prescribed sequences of discourse elements . . . ; (f) When students are given structurally relevant sentence openers to choose from ("On the other hand, . . ." "Another reason, . . ." etc.), they claim that this makes composition easier, although we have not been able to detect any influence on their compositions . . . , which suggests that these cues feed into their existing strategies; (g) The hypothesized strategy is consistent with the scarcity of substantive revision in children's writing . . . ; (h) A similar topic-based strategy has been identified in children's reading comprehension . . . . (Marlene Scardemalia and Carl Bereiter, "Assimilative Processes in Composition Planning," Educational Psychologist, 17 [Fall 1982], p. 166)

In recent years cognitive psychologists have been carrying on a full debate--with all the available evidence--over the use of verbal reports as data and have sorted through many of the problems that still seem to trouble the critics I have been quoting. Two excellent sources for a review of both sides of the debate (and the abstracts of those papers) are:

Telling More Than We Can Know:
Verbal Reports on Mental Processes

Richard E. Nisbett and Timothy DeCamp Wilson
University of Michigan

Evidence is reviewed which suggests that there may be little or no direct introspective access to higher order cognitive processes. Subjects are sometimes (a) unaware of the existence of a stimulus that importantly influenced a response, (b) unaware of the existence of the response, and (c) unaware that the stimulus has affected the response. It is proposed that when people attempt to report on their cognitive processes, that is, on the processes mediating the effects of a stimulus on a response, they do not do so on the basis of any true introspection. Instead, their reports are based on a priori, implicit causal theories, or judgements about the extent to which a particular stimulus is a plausible cause of a given response. This suggests that though people may not be able to observe directly their cognitive processes, they will sometimes be able to report accurately about them. Accurate reports will occur when influential stimuli are salient and are plausible causes of the responses they produce, and

will not occur when stimuli are not salient or are not plausible causes. (Psychological Review, 84 [May 1977], pp. 231-258)

Verbal Reports as Data

K. Anders Ericsson and Herbert A. Simon
Carnegie-Mellon University

The central proposal of this article is that verbal reports are data. Accounting for verbal reports, as for other kinds of data, requires explication of the mechanisms by which the reports are generated, and the ways in which they are sensitive to experimental factors (instructions, tasks, etc.). Within the theoretical framework of human information processing, we discuss different types of processes underlying verbalization and present a model of how subjects, in response to an instruction to think aloud, verbalize information that they are attending to in short-term memory (STM). Verbalizing information is shown to affect cognitive processes only if the instructions require verbalization of information that would not otherwise be attended to. From an analysis of what would be in STM at the time of report, the model predicts what can reliably be reported. The inaccurate reports found by other research are shown to result from requesting information that was never directly heeded, thus forcing subjects to infer rather than remember their mental processes. (Psychological Review, 87 [May 1980], pp. 215-251)

A considerably expanded version of the Ericsson and Simon paper has appeared as a book: K. Anders Ericsson and Herbert A. Simon, Protocol Analysis, Verbal Reports as Data (Cambridge: MIT Press, 1984).

There is not space here to give the evidence in support of these generalizations or to say much more about these sixty-five pages of discussion beyond the abstracts I have given. I would like to offer two further quotations, however, in support of the case I have been making. First, although, as the abstract indicates, Nisbett and Wilson claim that "There may be little or no direct introspective access to higher order cognitive process" (p. 231), they do report research which they summarize as follows:

> The individual knows a host of personal historical facts; he knows the focus of his attention at any given point in time; he knows what his current sensations are and has what almost all psychologists and philosophers would assert to be "knowledge" at least quantitatively superior to that of observers concerning his emotions, evaluations, and plans. (p. 255)

The critical phrases here are: "he knows the focus of his attention at any given point in time"

and "he . . . has . . . 'knowledge' at least quantitatively superior to that of observers concerning his . . . evaluations, and plans." Protocols seek to capture: the focus of a writer's attention from moment to moment; and traces of the writer's evaluation of what he or she has written from moment to moment and of his or her plans as the planning process proceeds before and during the writing.

Second, I should like to quote the Ericsson and Simon (1980) summary evaluation of the Nisbett and Wilson paper:

> A well-known review article by Nisbett and Wilson (1977) is sometimes thought to provide evidence discrediting verbal reports as data. A close examination of the specific studies analyzed in their article shows that the instances cited of inconsistency between verbal reports and data all refer to experimental situations and procedures where our model would predict that veridical reports could hardly be expected. In fact, Nisbett and Wilson's own detailed summary of the conditions under which verbal reports can be assumed to be valid are consistent with the conclusions we have reached in this article. (p. 247)

If my statements about the two papers and my quotations from them have not persuaded some of the readers of this paper, I hope, at least, that those who are not persuaded will read the two papers themselves and examine the evidence. Certainly no one should presume to pass judgment on protocol analysis as a method for examining the writing process without doing so. Both papers were available before any of the papers critical of protocol analysis that I have quoted were written: the first one appeared in May 1977, the second in May 1980; all of the critical articles I quoted appeared in 1983. The fact that none of the critics of protocol analysis I have quoted seems to have been aware of them says something, I think, about the care with which they looked into the problem.

There are two final points to be dealt with. First, in discussions of protocol analysis, critics frequently question whether such methods are "scientific." Connors, for example, raises that issue in "Composition Studies and Science," referred to earlier. Fortunately for his reader, he gives the definition of science that he uses as a criterion:

> There is some disagreement among philosophers of science about what "gate criteria" should be, but among the areas of general agreement are the following:
>
> 1. The aims of a scientific field are description, explanation, and prediction.
>
> 2. Scientific fields are marked by a naturalist metaphysics and an empiricist methodology.

> 3. Hypotheses in scientific fields should be falsifiable and should result in successful predictions, the success of which should be explicable.
>
> 4. The method of choice in scientific fields is the experiment, which allows phenomena to be disembedded from their contexts and analyzed by being ordered to formal and mathematical systems.
>
> 5. The mark of success in a scientific field is cumulative progress toward the aims stated above. If this is not absolute cumulation, it must at least be progress to the point of demarcating soluble puzzles within the discipline from mere problems. (pp. 6-7)

Note the requirement of "the experiment" (item 4). I assume he means here what Deese, whom Connors quotes, calls "the classical experimental model" or "experimentation in the classical sense": i.e., control and experimental groups, dependent and independent variables; or, as Deese puts it, "the model of experimenting taken from the physical sciences" (p. 522).

By Connors' standards, then, protocol taking and analysis probably is not scientific. But listen again to Deese, who says, "[W]e have been badly oversold on the classical experimental model as the means of studying such central aspects of human psychology as cognition. Too much evidence--obvious and available evidence--is ignored because it fails to conform to our prejudices about how empirical information arises" (p. 518): "empirical information," as in Connors' item 2, above. And I have already quoted Deese's statement that "That does not mean that we cannot apply the tools of science, only that we should consider whether the particular tools being used serve any genuinely useful purpose (p. 518). Thus Deese does not think that rejecting "the classical experimental model" because it is fruitless for cognitive psychology means that he will no longer be a scientist. Indeed, Deese, one of Connors' own witnesses, sees Connors' requirement number 4 as a prejudice.

Certainly many cognitive psychologists think of themselves as scientists. Here, for example, is the opening paragraph of a statement which appears on the inside front cover of the series of occasional papers published by the Centre for Applied Cognitive Science of The Ontario Institute for Studies in Education (OISE):

> Cognitive science is a relatively new interdisciplinary field drawing on cognitive psychology, computer science, and linguistics; the attraction of cognitive scientists from several diverse specialities creates a collective expertise that enhances information exchange and research capabilities. The primary impetus for a cognitive science treatment of any problem is the realization that it is possible to specify a set of rules which are the basis of any intelligent performance. Well-known in the fields of

> computer science and linguistics, this specification of rules has been found to be a powerful means of analyzing any psychological performance.

Clearly the psychologists at OISE think of cognitive psychology as a science.

If one insists on the "classical experimental model," then Charles Darwin was not a scientist. Many other kinds of scientists deal with phenomena whose effects they see only indirectly and on which they can not perform "experiments in the classical sense": seismologists and geologists who deal with strata of the earth that they have never seen, for example, and physicists who can't see quarks directly, but must rely on their traces for evidence.

Let us look at the problem another way. Examining the final product of the writing process, an essay, for example, or a novel, tells us little about the process. Looking at successive drafts of a piece of writing does tell us more. For example, by examining the changes that James Joyce made in the manuscript of Ulysses, I was able to determine that Joyce

> strengthened the distinctive elements of Bloom's stream of consciousness, sentence patterns found typically in Lestrygonians [chapter 8 of Ulysses] but not in Proteus [chapter 3] . . . . Had Joyce not added these sentences in revision or had he removed sentences of this pattern instead of adding them, Lestrygonians would have been much less distinguishable from Proteus than it is. By that much, therefore, Bloom would have been less distinguishable from Stephen. (Erwin R. Steinberg, The Stream of Consciousness and Beyond in ULYSSES [Pittsburgh: University of Pittsburgh Press, 1973], p. 109)

Was that a conscious stratagem on Joyce's part? We don't know. Had Joyce been talking into a tape recorder as he made those changes, we would know more. Would we know everything about Joyce's process of writing that we might want to know? Probably not. But we would know enough to begin to build a more reasonable model of Joyce's writing process than we do now. The more traces of the process itself that we have, the better we can understand the process.

It is not unusual, of course, for literary scholars to attempt to discover a writer's writing process. Typically a scholar will try to determine process by examining successive drafts of product. Gerald Langford, for example, tried to determine "whether [Faulkner's] Sanctuary was a careless potboiler turned into a respectable book through reworking" by comparing the unrevised galleys and the published book (Faulkner's Revision of SANCTUARY [Austin: University of Texas, 1972], p. 5). He also considered two retrospective comments by Faulkner: the first the written Introduction to the Modern Library edition of Sanctuary which appeared the year after the

novel was originally published (p. 3), and the second, oral comments that Faulkner made at the University of Virginia, long after he wrote the book (pp. 3-4). Even before he quotes them, however, he comments on the "doubtful value of [such] author's statements" (p. 3).

How much more useful would have been a tape recording of a talking-aloud protocol of Faulkner as he made the significant changes in his manuscript--a concurrent verbalization of the changes he was making and the reasons he was making them. Such a protocol in addition to successive drafts of a novel would be very helpful--particularly if they show crossings out and marginal and interlinear additions as many of the Virginia Woolf and James Joyce holographs, typescripts, or galleys do. As the research has shown, concurrent verbalization is much more reliable than retrospective reporting.

Currently, of course, protocol analysts have these several traces available to them in tracking the writing process: the concurrent verbalization; and the product, with all of its crossings out, additions, and even doodles.

The reader may have noticed that in the two paragraphs above, I have characterized protocols as "useful," "helpful," and "reliable." But are they "scientific"? At best that question is ambiguous. They are scientific or not depending on how one defines scientific. Clearly, as I have indicated above, by Connors' criteria they are not, but by Deese's they are.

Ultimately, however, whether one labels protocol analysis scientific or not is irrelevant. What does one mean by "scientific evidence," anyhow? That it is irrefutable? Of course not. Many things that scientists once "knew," contemporary scientists "know" not to be true. That is one of the important points of Thomas Kuhn's The Structure of Scientific Revolutions (2nd ed.: Chicago: University of Chicago Press, 1970), which Connors examines at some length at the beginning of his paper. Accepting the label "scientific" on a method does not relieve one of the responsibility of looking at the evidence and weighing it.

Scientists or not, we must use the tools we have. Let the protocol analysts take their protocols, analyze them, and build models from them. If their evidence and their models seem reasonable, we can test the models in our research and teaching. (Perhaps at that point it may even be appropriate to use "the classical experimental model"!) Surely even an approximate

model will be at least as useful--and perhaps less harmful--than some of the fads we've subscribed to in the thirty years or so that we've had a Conference on College Composition and Communication.

Finally, a vexing part of the problem in dealing with the criticism of protocol analysis and the emphasis on process is that the critics of the methodology don't agree with each other on what is wrong with it; and, that after mounting an attack, they sometimes back off, part way or even all the way. Thus for example, one critic who seems concerned that research is emphasizing process at the expense of examining product says:

> The writing process is obviously linear. We have to put one thing after another--one letter, one word, one sentence, one paragraph after another. The observation is so obvious that we tend to overlook it, but by thinking of prose as a sequence we can get new insights into ways of generating ideas and keeping them in order. (Robert M. Gorrell, "How to Make Mulligan Stew: Process and Product Again," College Composition and Communication 34 [October 1983], p. 275)

A few pages later in the same issue, another pair of critics say, "Yes, certainly writing is a recursive, complex process" (Marilyn Cooper and Michael Holzman, p. 292). Yes, the writing process is recursive; the product is linear. We do "have to put one thing after another--one letter, one word, one sentence, one paragraph after another." But we are always circling back: we plan, we write, we plan, we write, we edit, we write, we plan, we reread, we plan, we write . . . . The evidence from protocol studies is so clear on that that one needn't even footnote the statement (and I won't).

The same critics who agree that writing is a process, however, and who even agree that "cognitive psychology's models of scripts, plans, and goals may help us understand this process" and "that Flower and Hayes have taken pioneer steps" worry, nevertheless, that the Flower/Hayes research methodology is not "ecologically valid" or "replicable" and conclude that "their plans seem vague and their scripts not those we wish to encourage" (Cooper and Holzman, p. 292).

> Still in that same issue, another critic trusts process-tracing methodology a little farther:
>
> Even with their empirical limitations, the process studies by Emig and her successors, and the theoretical formulations that they have ultimately made possible, have been useful to us. Emig's own delineation of ten "dimensions of the composing process" (Chapter 3), and such formulations as John R. Hayes' and Linda Flowers' "Cognitive Process Theory of Writing" (CCC, December, 1981) have given us much more general understanding of the essentially vitalistic business of how human beings write. This general understanding can, increasingly does, and ought to inform our pedagogical

practices.

"But," he continues, "it is doubtful that research on composing processes, particularly that research using 'composing aloud' tapings, will give us much more in the way of really usable information" (Voss, pp. 282-283). It seems that adequate "precision . . . is not possible" in what he characterizes as "social science research"--precise enough seemingly, to accept what has been done so far, but that's far enough, thank you. Research on the composing process, he says, has committed the sin of giving "the misleading impression . . . that the methods of science have given us much exact knowledge of students' composing processes" (p. 282).

The same year in another NCTE journal, yet another critic also objects to the presumptions of process-tracing methodology to being "scientific," objects at considerable length: eighteen pages! Along the way, however, he points out in a footnote that it is not the researchers themselves who make these claims, but "those who are not themselves engaged in empirical research" (Connors, p. 11) and that

> I am not here arguing--and I wish to be very clear about this--that there should be no empirical research in composition studies, that we should attempt no rigorous analyses or descriptions of phenomena, or that we should not strive toward a rational and skeptical perspective on our body of knowledge . . . . We cannot cut our discipline off from verifiable knowledge . . . . Empirical research has much to teach us . . . . (pp. 19-20)

The empiricists buried in the footnote on page eleven are thus not only absolved, but, finally, redeemed.

There are, perhaps, shortcomings and even possible dangers to some of the empirical research on the writing process that has been undertaken (Erwin R. Steinberg, "A Garden of Opportunities and a Thicket of Dangers," in Cognitive Processes in Writing, ed. Lee W. Gregg and Erwin R. Steinberg [Hillside, NJ: Erlbaum, 1980], pp. 155-167). But there are even greater dangers inherent in taking blunt rhetorical instruments to poorly understood, delicate new machinery. Empiricism should not be seen as a threat by humanists. Humanists would do well to study the empirical techniques available from the social sciences and from computer science--including protocol analysis. Many of them are useful in humanistic research. Furthermore, if one wants to argue against some of them, one should understand them first in if one is to argue intelligently and persuasively.

I can find no better way to conclude this paper than to quote the conclusion of the Ericsson and Simon paper (1980):

> For more than half a century, and as the result of an unjustified extrapolation of a justified challenge to a particular mode of verbal reporting (introspection), the verbal reports of human subjects have been thought suspect as a source of evidence about cognitive processes. In this article we have undertaken to show that verbal reports, elicited with care and interpreted with full understanding of the circumstances under which they were obtained, are a valuable and thoroughly reliable source of information about cognitive processes. It is time to abandon the careless charge of "introspection" as a means for disparaging such data. They describe human behavior that is as readily interpreted as any other human behavior. To omit them when we are carrying the "chain and transit of objective measurement" is only to mark as terra incognita large areas on the map of human cognition that we know perfectly well how to survey. (p. 247)

Let those who want to disagree demonstrate otherwise. Anyone who wants to know how many teeth horses have should look in the mouths of a reasonable sample of horses. We shake our heads sadly over the antics of the disputants some centuries back who preferred to debate the matter rather than look. But we undertake analogous debates ourselves.

[1]I am indebted to my colleague Richard E. Young for comments on an earlier draft of this paper.